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THE RISE OF GENETICS[1]

By Professor T. H. MORGAN

CALIFORNIA INSTITUTE OF TECHNOLOGY

THE new developments in science that occur from time to time can generally be traced either to the invention of a new method or to the discovery of a new fact that has far-reaching consequences, or to the elaboration of a new theoretical principle that suggests new lines of investigation. In the latter case, it is the prerogative of science, in comparison with the speculative procedure of philosophy and metaphysics, to cherish those theories that can be given an experimental verification and to disregard the rest, not because they are wrong, but because they are useless.

In the case of genetics the situation was in some respects different from any of these procedures; for it began with the discovery of a discovery that had been made 35 years before. We can date the beginning of genetics, then, from the resurrection of Mendel's paper in 1900. Its rehabilitation was not, however, due to a literary find, but to a need resulting from similar experiments by de Vries, Correns and Tschermak that unveiled a series of phenomena identical with the facts of Mendel's earlier work.

The significant fact is that when the time was ripe to appreciate its fundamental significance, Mendel's forgotten paper was discovered with the result that the activities of hundreds of biologists, as the program of this present Congress bears witness, had the direction of their scientific careers entirely redirected, or begun along new lines. The discoveries that rapidly followed, showing that the same laws applied widely to the other plants and to animals also, brought about realization that a great step forward in biology had been made.

But before we consider the rise of genetics after the year 1900, it is proper on this occasion to pay

[1] Address of the president of the Sixth International Congress of Genetics at Cornell University, Ithaca, New York, August 25, 1932.

tribute to the earlier work in hybridizing that furnished the background of procedure to which Mendel himself probably owed a considerable debt. Let us pause for a moment and recall a bit of history, for it would be unfair to forget or to underrate everything prior to the first year of the present century.

If to-day we express surprise that Mendel's paper remained unnoticed for 35 years, let us recall that this is a not unfamiliar experience in biological science. Between the experimental proof of sex in plants by Camerarius (1694) and the prize essay of Linnaeus on the sex of plants (1760) sixty-six years elapsed.

At about this time the scientific study of hybridizing may also be said to have been begun by Linnaeus and his students, and especially by Kohlreuter in several memorable papers (1760–66).

Then, thirty-three years elapsed before Sprengel's (1793) observations on the natural crossing of plants by insects, which made clear that cross-fertilization is of wide-spread occurrence in flowering plants.

More interesting, perhaps, to modern geneticists are the pioneer experiments on peas that, in a very real sense, were the precursors of Mendel's work. It is not as generally known as it should be that some of the facts on which Mendel's results with garden peas rested had been recorded by several earlier experimenters. In 1823 Thomas Knight, 42 years before Mendel, described a cross between a pea with a gray seed-coat and one with a white that gave seeds which were uniformly gray-coated. These seeds when grown produced in the next year both gray and white seeds. John Goss had in 1822 also reported experiments with garden peas and found the first generation of offspring had seeds like the paternal race. From these in the next generation he obtained peas of two kinds, one like those of the original grandpaternal race, the others like those of the grandmaternal. Separating these he found that the blue peas produced in F_3 only blues, and the white peas both blues and whites. Here is an example of what to-day we call dominance and recessiveness, as well as segregation in F_2. In the same year (1822) Alexander Seton reported similar results. Nearly fifty years later Thomas Laxton (1868–72), working with peas, recorded numerous facts similar to those first spoken of, and in addition he mentioned cases in which two pairs of contrasted characters were present. Assortment between the pairs was found—which result is familiar to students to-day and which Mendel's work established.

Amongst the earlier hybridists the name of Naudin (1861–64) is most often referred to as a forerunner of Mendel, and it is sometimes stated that he anticipated Mendel's discoveries. His principal prize paper appeared in 1863, two years prior to Mendel's paper before the Brunn Society, and was followed by two others in 1864 and 1865. Naudin laid emphasis on the identity of individuals of the first generation hybrids, including reciprocal crosses. He insisted on the intermediate character of the F_1 hybrid, with the important reservation that the intermediate forms do not stand always equally distant from the two parents. We now know that, taken character by character, sometimes an intermediate condition, sometimes complete dominance, may be found. But whichever condition holds for a particular character, the phenomenon of segregation in the germ-cells of the F_1 hybrid remains unaffected.

Naudin stated explicitly that in the second and later generations there is a mixture of forms, including some which are like the original parents and others that approach these in various degrees. Then follows his most important deduction, namely, that the second generation results find their explanation in the disjunction of the two specific essences derived from the parents in the ovules and in the pollen of the hybrid. Here we have a highly significant contribution, for, not only did Naudin see clearly that the results are explicable on the principle of disjunction (or, as we say now, segregation), but that this, taking place both in the egg and in the pollen, gives the kinds of characters that appear. So important historically is this fact that there should be included his specific statement showing that he had a perfectly clear idea as to how disjunction accounts for the diversity in the second generation. If, he says, a pollen grain bearing the characters of the male parent meets an egg of the same kind, a plant that is a reversion to the paternal species will result; similarly for the maternal species. But if a pollen grain of one kind meets an egg of the other kind, a true cross-fertilization takes place like that of the first generation and an intermediate form will result. It will be agreed, I think, on all hands that this was a brilliant interpretation of results based on first-hand experience. It falls short of Mendel's work in two or three important aspects: (1) The failure to put the hypothesis to a test by back-crossing; (2) the failure to see what the numerical results should be on the basis of disjunction of the elements in the hybrid. His use of the words "disordered variation" in the F_2 and later generations brings out the essential difference between Naudin and Mendel. It is the orderly result of disjunction or segregation that is the important feature of Mendel's work; and finally, the clearness with which Mendel stated and proved the interrelation between character-pairs in inheritance, when more than one pair is involved, places his work distinctly above everything that had gone before. Nevertheless, the genial abbot's work was not entirely heaven-born, but had a back-

ground of one hundred years of substantial progress that made it possible for his genius to develop to its full measure.

If, in this brief review, I have neglected to bring in the names of a number of well-known selectionists whose work has been in the main in the field of agriculture, it is not because I do not realize the importance of their work or the great difficulties they overcame, but because, for the moment, we are interested especially in the development of our theoretical knowledge of genetics.

So far I have spoken only of plants. What part, may be asked, has the study of animals played in the pre-Mendelian history of genetics, *i.e.*, down to 1865?

The question of sex in plants that took botanists a hundred years to decipher was not so difficult for zoologists. If we may accept the traditional story, it was not unknown in the Garden of Eden. Aristotle had a good deal to say about it. The credit of finding a sex-determining mechanism can properly be claimed by zoologists, but this happened only in the opening years of the present century.

Hybridizing was also familiar to zoologists, but in pre-Mendelian times occupied only a relatively small part of their interest. What was known has been recorded by Darwin in his "Animals and Plants under Domestication." This scattered and loose information was incorporated after 1859 in the discussions of the theory of evolution.

The chief contribution of zoologists to present-day genetics was along different lines. In the latter half of the last century there was great activity in the field of cellular morphology. The important facts concerning chromosome-division and the extraordinary changes that take place at the time of maturation of the germ-cells and at fertilization were first made out by zoologists. The names of Kölliker, Flemming, Fol, Van Beneden, Hertwig and Boveri are landmarks in the history of cytology. Correspondingly for plants the names of Hofmeister, Strasburger, Du Bary and Guinard run a parallel course.

Weismann's theoretical contributions have also played an important historical rôle. "The Continuity of the Germ-plasm" served to counteract the all-too-prevalent influence of Lamarck and his successors, whose views if correct would undermine all that Mendel's principles have taught us. Weismann's speculations on the origin of new variations by recombination of elements in the chromosomes, while not to-day acceptable as stated by him, nevertheless focused attention on an important subject. His discussion of the interpretation of the maturation divisions played, I believe, a leading rôle in directing attention to a subject that was destined very soon to have great importance for genetics.

Thus at the end of the last century some extraordinary advances had been made in unraveling the changes that take place in the maturation of the germ-cells. These advances led to the recognition of a mechanism that was to place the theoretical elements of Mendel's hypothesis on a firm foundation of fact. But this, however, was not apparent until 1903.

Genetics at the Beginning of the Century

We come now to the fateful year 1900, when three lines of fundamental significance for genetics were ready to be brought together. I refer, of course, to the mutation theory of de Vries, to the rediscovery of Mendel's paper, and to the application of the discoveries in cytology to the new theories.

The intimate connection between the mutation theory, as first propounded, and the origin of the characters that follow Mendel's laws was not immediately evident, since de Vries laid emphasis on the many character changes that result from each progressive mutational step. In fact, at about this time de Vries recognized three types of mutational changes: Progressive changes—changes that introduce something new, leading to the sudden appearance of a new elementary species; retrogressive changes, the result of something lost or becoming latent; and degressive changes, in which old characters are revived.

This nomenclature, in so far as it is purely descriptive and based on characters rather than changes in the germ-plasm, covers broadly many of the facts with which we are familiar to-day. But in the light of the work of the last 30 years, especially when applied to genes, this description can no longer be accepted as fundamental; for now we have information that gives a more consistent picture of the changes produced by the genes. For example, the evidence from hybridizing elementary species, on which de Vries based in part his distinctions, has to-day a different interpretation. We no longer hold that a progressive change introduces an entirely new, unpaired element into the germ-track, for the unpaired chromosome in cases of heteroploidy can surely not be regarded as the usual step for progressive evolution. Again, the permanence of certain hybrid combinations, whenever such exceptional cases arise, are not now regarded as due to the introduction from each parent of a new unpaired element, but can be interpreted in different ways in different cases.

It was the emphasis that de Vries laid on mutational changes in the germinal material as sharply discontinuous, irrespective of the effect on the character, that has had important and far-reaching consequences for genetic work and theory.

The groundwork for discontinuous phenotypic

variation had in 1894 been laid by Bateson's contribution on discontinuous variation. While we recognize that some of the examples Bateson collected are not inherited but phenotypic (which confused the picture), nevertheless his insistence on the importance of discontinuity prepared the way for the acceptance of the more fundamental distinction that de Vries had made.

But I wish to emphasize that the revolution in our ideas that took place at this time was not so much due to the insistence on discontinuity of somatic structures, but discontinuity in the hereditary elements. An example will serve to illustrate the difference. When a gene changes, its effects on new characters, taken individually, are generally very different. Some of them may be sharply marked off from the original character. The character showing the greatest effect is the one generally picked out for genetic work. But at the same time there are changes in other organs that are less conspicuous—some of the characters are so little affected or so variable that, taken by themselves, they would give a picture of continuity rather than of discontinuity. They would often pass unnoticed were not attention drawn to them by the discovery of the major change.

For the theory of evolution some of these inconspicuous changes may be more significant than the more obvious discontinuous change. In fact, if evolutionary advances are more often through invisible physiological mutational changes rather than morphological ones, we can better understand the paradoxical situation in which taxonomists find themselves, to wit, that the sharp structural differences, that are used for diagnostic separation of species, relate to characters that seem often to be unimportant for the well-being of the individual. The new point of view is a complete inversion of much of the thinking in which the evolutionary theory indulged in the past.

As I have said, the rapid expansion of genetics after 1900 has been intimately connected with the applications of the chromosome theory to the experimental work in genetics. The integrity of the chromosomes and their continuity from one cell-generation to the next, the constancy in number of the chromosomes in each species and the absence of mixing of the materials of the conjugating chromosomes at the time of meiosis have furnished the basis on which genetics rests.

I think we can not overemphasize the significance of this relation between the theoretical side of genetics and the factual side as observed in the known behavior of the material basis of heredity. To put the matter bluntly, the recognition that there is a mechanism to which genetic theory must conform, if it is to be productive, serves to keep us on the right track and acts as a check to irresponsible speculation, however attractive it may seem in print.

Some one may reply that it is not always an advantage to keep one's nose to the grindstone. Granted! but realizing how often ingenious speculation in the complex biological world has led nowhere and how often the real advances in biology as well as in chemistry, physics and astronomy have kept within the bounds of mechanistic interpretation, we geneticists should rejoice, even with our noses on the grindstone (which means both eyes on the objectives), that we have at command an additional means of testing whatever original ideas pop into our heads.

Expansion Since 1900

I come now to the expansion of the Mendelian theory that has taken place in the last 30 years. If I refrain from giving the names of the numerous contributors to this advance, it is because many of the discoverers are before me in person; or, if not, will get reports of the congress. Future congresses will probably be better able to evaluate individually the merits of those who have made the significant contributions in this generation.

It must have been evident to many geneticists after 1903 that if the chromosomes are the bearers of Mendel's elements, there would be only as many independent characters as there are chromosomes, provided the then current idea of the integrity of the chromosomes were true. This would place limitations on Mendel's second law—the law of independent assortment. In fact, the genetic evidence can now be said to have firmly established that there are more characters independently inherited, within stated limits, than there are chromosomes.

But linkage also turned out to have its limitations, and these very limitations made it possible to determine the localization of the genes in the chromosomes. I refer, of course, to crossing over. Since localization of the genes is to-day the basis of much of the quantitative work in genetics, I may be allowed to elaborate it somewhat.

The outstanding genetic fact is that these interchanges take place only between homologous chromosomes—*i.e.*, between members of the same pair.

The second important genetic fact is that when the interchange takes place, large blocks of the chromosomes are exchanged. This can be proven only in cases where more than two loci are involved, and best when a considerable number of well-spaced genes have been located. Until recently the evidence that large blocks of genes are involved in crossing over was known only genetically. No certain cytological proof was known. To-day, however, the proof has

been found. No doubt this cytological evidence will be presented and discussed at this congress.

It has also been determined on genetic evidence that more than one interchange may take place between a pair of chromosomes, which can be checked only in cases where there are enough intermediate loci between two pairs to serve as markers.

A moment ago I said that crossing over has furnished the basis for the theory of localization. May I give an illustration, in the hope of removing a criticism of the localization technique that is based, I believe, on a misunderstanding? It has been said, for example, that the changes made from time to time in the genetic map of the Drosophila chromosomes discredit the method by which the localization is determined. It might as well be said that the method by which the atomic weights in chemistry were gradually improved discredited the procedure of the chemist.

Two illustrations will serve our present purpose. Let us suppose a new mutant character is found and its chromosome group—*ie.*, its linkage group—determined by familiar methods. We may proceed, then, to find its relation to two known loci in that chromosome. If these are far apart, the cross-over data will give only its approximate position. Having found this, it may turn out that the locus lies near another gene in that region, but whether above or below may be uncertain. We next proceed to find its more exact location with respect to this third gene, using either of the other two genes as a second point. In this way the new gene is more accurately placed with respect to the third locus. Further work will then be necessary if there are other genes in this region.

The second illustration concerns a distinction between crossing-over data given in the actual experiment and its conversion into map distance. For very small values, say 5 points, the two are the same because double crossing over is not present. But in longer distances the crossing-over data may depart widely from the map figures because double crossing over makes the figures too low. In Drosophila, for example, the sex chromosome is 70 units of map distance, but the crossing-over data are found to give not over 50 units. In this case the map distance has been built up piece by piece through the summation of crossing-over data of loci so near each other that double crossing over is eliminated.

In other animals and plants, where few loci have as yet been found, the cross-over data are generally put down as map distance. This may be far from the real map distance, and since the actual amount of double crossing over in such less-worked-out forms is unknown, and since crossing over is different in different species, the loci must be regarded as only provisional.

There is another factor to be taken into account. The theory of localization was based in a general way on the assumption that crossing over in one region of a chromosome has the same frequency as in other regions. The Drosophila workers have long known that this is not exact, and in fact they had invented methods to show that crossing over is different in different regions.

The crowding of the genes in some regions of the genetic map and their scarcity in other regions has been shown to be due to the different frequencies of crossing over per unit of absolute distance in the cytological chromosome. This seems to be a fundamental relation for all chromosomes. In the X-chromosome of Drosophila, which appears to be a special case, most of the genes are crowded at the two ends of the chromosome with a middle region of undetermined length having few or no genes, in the sense that the Y-chromosome is empty of genes. These facts do not invalidate the purpose for which the maps were invented, since the relative position of the genes remains the same. It is their position relative to each other, allowing very precise prediction of the topographical relation of a new gene to all other known genes, as determined by corrected cross-over data, that is important.

This brings us to one of the most recent fields of modern genetics—the study of the redistribution of the linkage group by translocation. Treatment with x-rays has been found to be a prolific source for material of this kind, but it should not be forgotten that translocation had been discovered and utilized for genetic interpretation several years before x-rays were used. Even to-day, with much evidence before us, the way in which x-rays bring about this result puzzles us. In a crude way we might picture the electron shooting holes in the chromosomes, thus breaking them apart. But when the relative sizes of the electron and the chromosome are considered, it is difficult to see how such a disruption would result from a single shot.

Even more surprising is the fact that the broken end of a piece may reunite with the end of some other chromosome and, acquiring thereby an attachment fiber, form a new linkage group. Of course it does not follow that such a reunion occurs whenever a chromosome is broken. It is only those cases where reunion does occur that are recovered and studied by geneticists. When no such union is brought about the piece, lacking an attachment point, will be lost, and the zygote containing it will probably die.

As I have said, the astonishing fact remains that

the broken end becomes at times attached to the end of another chromosome. Without the objective evidence of this union that we have to-day, it might have been supposed that the broken-off fragment would rather have made, or retained, a side-to-side union with a corresponding part of its homologous chromosome. However, since the conditions of the cell that permit conjugation of like chromosomes occurs only once in the life-cycle, such a union is not, then, to be expected if the breaking has occurred after that event. If it had occurred earlier in the germ-track the piece would no doubt have been lost before meiosis came on. Here, then, we have a field inviting speculation. Let us hope that it will not long remain there, but that evidence concerning these puzzling relations may soon be forthcoming.

In this connection I need hardly recall to mind that, on the current theory of crossing over, the linear order of the genes is broken at the same level in two of the strands, and a new lengthwise reunion of the broken ends takes place. Whether this breaking and reunion is a comparable process to that seen in translocation we do not know, and it would be unprofitable at present even to make guesses.

POLYPLOIDY

In even a passing review of present-day genetics, the numerous problems connected with the increase in number of the chromosomes, or polyploidy in technical language, can not be ignored. But how can one hope even to summarize the work that is pouring in with the arrival of every new number of the genetics journals? The importance of polyploidy for the evolution doctrine is perhaps clear, but needs cautious handling in the light of the past history of phylogenetic interpretation of the facts of comparative anatomy. I hope that that history, at least, will not be repeated when the story of genetics comes to be written, for, in the light of recent work on the exchange of limbs between non-homologous chromosomes, and on translocations, the comparison of chromosome numbers without this knowledge may be very misleading. The determination of the linkages of the genes is the only safe basis for such comparisons.

At present I can do no more than briefly indicate some of the obvious and salient points. In many families of plants, and also in a more limited number of animals, chromosome groups are present that are multiples of a basal number, usually of the haploid number of the lowest member of the group. These are frequently double or triple, or quadruple groups of a basal number, generally assumed to be the haploid number. A good many of our cultivated plants are also known to show multiples of a real or postulated basal number of chromosomes. It is natural to assume that, in many cases, this has come about by the actual doubling of the whole chromosome group rather than by breaking of the chromosomes, that would also lead to doubling their number. It is more consistent to assume that doubling is the method by which the number of chromosomes is increased, because of the evidence from the sizes of the chromosomes, from their method of conjugation and from the relation of chromosomes to the attachment-fiber.

There are several known ways in which we can bring about a doubling of the number of chromosomes in a cell. The usual way is to suppress the cytoplasmic division of the cell at the time when the chromosomes divide. When this is done the chromosomes do not reunite, but the descendants of that cell will forever possess twice the original number of chromosomes. Theoretically, the process might go on forever, unless there are upper limits of a physiological nature preventing an indefinite increase. Doubling of diploids gives tetraploids. These crossed to diploids give triploids. Double tetraploids (or octoploids) crossed to tetraploids give hexaploids, and so on.

This work furnishes an opportunity for the solution of certain genetic problems of theoretical interest, for, without this knowledge, some of the known genetic ratios would have been difficult to interpret. With this knowledge they are found to conform to recognizable general principles.

It is perhaps ungracious to point out that the mere study of chromosome numbers in different species may in itself become mere hackwork. It looks as though it may become as popular for academic work as section-cutting of embryos was at an earier period. It is more generous, perhaps, to regard the work on chromosome counts as pioneering, and therefore preliminary work in the search for new materials, some of which will certainly be of value for deeper-lying genetic problems. This is especially evident in the study of hybrids whose parents, whether cultivated or wild types, have different numbers of chromosomes. The erratic behavior of the chromosomes, often seen in the maturation of the germ-cells of such hybrids, clearly explains the exceptional and often abnormal results that follow. Without this information we might be tempted to indulge in much profitless and arbitrary speculation.

Not only are we familiar with cases where a multiplication of the same group of chromosomes is brought about within the species, but there are a few cases where an increase has been brought about by crossing distinct species with different numbers of chromosomes, and chromosomes that do not mate at meioses. These situations are full of interest for

students of genetics, presenting a wide range of new possibilities.

Of great importance for the genetic interpretation of polyploidy in terms of chromosomes is the identification of chromosomes that carry specific genes. Only a few years ago this was known in only one animal, but the number of cases is steadily increasing. Until information of this kind becomes more general there will be, as at present, a good deal of guessing as to the relation of chromosome groups having different numbers of chromosomes.

(To be concluded)

SCIENTIFIC EVENTS

MUNICIPAL RECREATION AREAS

THE National Recreation Association has issued a statement in regard to recreation areas that have been donated to cities in the United States.

According to this statement park and recreation areas, valued at more than $100,000,000 and comprising 75,000 acres, have been donated to municipalities in the United States, according to a study made by the National Recreation Association.

These parks comprise one third of all municipal recreation areas, the remainder having been secured by the expenditure of municipal funds. Some cities, including New Brunswick, New Jersey; Oneonta, New York, and Raleigh, North Carolina, reported that every acre of their existing parks was secured through gifts.

Typical of the varied types of areas given are the Edwin Gould Playground of 6.5 acres in Dobbs Ferry, New York; Percy Warner Park of 700 acres in Nashville, Tennessee, and the Littauer Park and swimming pool of 4.1 acres in Gloversville, New York. H. C. Frick, of Pittsburgh, willed 151 acres, now known as Frick Park, to the city and in addition left a fund of $2,000,000, the income from which was to be used for maintaining, improving and adding to the park.

Northampton, Massachusetts, the home of former President Coolidge, was given an area of 103 acres, known as Look Memorial Park. A fund of $450,000 was also given by Mrs. Fannie B. Look for developing and maintaining the park. Fred Morgan Kirby in 1921 gave Wilkes-Barre, Pennsylvania, $250,000 to develop Kirby Park, adding $120,000 in 1924 and creating an endowment of half a million dollars.

A variety of motives inspired the park gifts. One applicable to many gifts is the desire to perpetuate the memory of a citizen who has given special service to his community or to the nation. In Summit, New Jersey, for example, the citizens, desiring to provide a lasting memorial to Hamilton Wright Mabie, made a fund to purchase and improve a tract of land opposite the Civic Center. The area, known as the Mabie Memorial Playground, possesses natural beauty and has been equipped with many recreation facilities, including tennis courts, a shelter house and playground apparatus. The Cauldwell Playground in Morristown, New Jersey, is a memorial to a former mayor, as is the David N. Ropes Playground in Orange of the same state.

Lieutenant Clayton C. Ingersoll Memorial Park of 110 acres in Rockford, Illinois, was given by the parents of the young man for whom the park was named. He was killed in the war.

Mr. and Mrs. William Allen White, of Emporia, have developed an area of fifty-one acres as a city park, naming it "Mary's Garden" in memory of their daughter. Among the restrictions accompanying the gift are that the name White will never be used in connection with the park and that the donors shall have five years in which to spend as much of their own money as they wish in improving the park.

Robert Greer Playground in Elizabeth, New Jersey, was given by the father of a boy killed by an automobile. The donor of Pope Park in Hartford, Connecticut, offered this area to the city, stating, "A large part of the success of any manufacturing business depends upon the health, happiness and orderly life of its employees." He made it a condition that the city buy another tract of land so situated as to benefit the employees of all the factories in that section of the city.

THE COST OF SCIENTIFIC JOURNALS

THE *Wistar Institute News* says: "On August 15, 1932, a circular letter was addressed to the editors of all journals published by The Wistar Institute. The responses from editors have been so gratifying in their support of the institute's policy that it seems advisable, in order to aid the editors in their arduous and thankless task, to publish the letter in *The Wistar Institute News*."

The letter, signed by Dr. M. J. Greenman, director of the institute, reads:

> During 1931, The Wistar Institute published more pages in its several journals than during any previous year (8,091 pages). During the first six months of 1932 there has been a very considerable increase in the number of pages over the first six months of 1931. At the same time the individual support of journals is decreasing.
>
> It would appear that some men who write papers are not interested in supporting journals. Perhaps there is a very good reason for this.

It is our impression that the cost of publication is a legitimate part of the cost of an investigation. A number of our leading laboratories are conducted on this policy.

In securing support for publications many factors must be considered. Some laboratories are unable to aid publications directly as they might do were it possible under the state laws.

The Wistar Institute is attempting to publish promptly all manuscripts accepted by the editors and secure for support of the several journals all possible income from their sale and lessen the cost of production by maintaining its own press.

The sudden increase in material to be published during the past few months, with a tendency on the part of authors to take no part in the support of the media through which their work is brought to the attention of their colleagues, causes us some anxiety.

Until we secure additional endowment for publication purposes it is necessary to secure aid from every possible source.

It is essential for us to request that for a time you accept only such papers as are of more than usual importance and that the illustrations be limited to those absolutely necessary, or permit us to ask authors to contribute toward the cost of illustrations, which in many cases exceeds the cost of text.

THE THERMO-HYGIENE LABORATORY

CONTRACTS have been let by the John B. Pierce Foundation for the erection of an experimental laboratory adjoining the Yale University School of Medicine, in which investigations will be conducted on ventilation, cooling and heating, and their effects upon human health and comfort. The laboratory is to be known as the Thermo-hygiene Laboratory. Research activities will be in charge of Dr. Leonard Greenburg and Dr. L. P. Herrington under the general supervision of Dr. C.-E. A. Winslow.

The building will be a three-story structure about fifty feet square, located on the corner of Congress and Liberty Streets. Construction is to be commenced at once and will be completed in about three months. Included in the equipment are two suspended experimental rooms, fifteen feet long, twelve feet wide and nine feet high, in which it will be possible to produce and maintain any desired temperature, humidity and air motion.

Professor Winslow, known as an authority on public health, has long been engaged in a study of the effect of atmospheric conditions upon health. He has been a member of the New York State Ventilation Commission since 1913, and is now abroad to make a survey of progress in Europe on problems of heating and ventilation. Dr. Greenburg, who is trained as an engineer and physician, has been a member of the U. S. Public Health Service for many years, and in this capacity has been working in the Yale laboratories on ventilation problems. Dr. Herrington is a psychologist, with special training in physiology. Both of these men are members of the Department of Public Health at Yale. Their cooperation with Professor Winslow will make it possible to approach from various specialized angles the problems to be studied in these laboratories.

The John B. Pierce Foundation was established in 1924 for the specific purpose of supporting research work "in connection with the sanitary and hygienic security of human beings and their habitations."

THE W. J. McDONALD OBSERVATORY

THE University of Texas has received by bequest of the late William J. McDonald, of Texas, a sum now slightly in excess of $840,000, to erect and maintain an astronomical observatory. The observatory will be known as "The W. J. McDonald Observatory of the University of Texas," and will cost, including site, buildings and equipment, approximately $375,000. The observatory will house an 80-inch reflecting telescope.

Construction will be undertaken immediately under the terms of a cooperative agreement between the University of Texas, which is to erect and maintain the observatory, and the University of Chicago, which is to provide the director and staff.

Dr. Otto Struve, who is director of the Yerkes Observatory of the University of Chicago, at Williams Bay, Wisconsin, is to become also director of the McDonald Observatory. The University of Chicago is to pay the salaries of Dr. Struve, an assistant director who will have a permanent residence at the McDonald Observatory, at least two observing assistants, and the necessary maintenance staff. As a result of the cooperative undertaking, plans of the University of Chicago to build an observatory and powerful reflecting telescope in one of the Southern states have been abandoned.

Following observations made this summer by an expedition headed by Dr. C. T. Elvey, a member of the Yerkes Observatory staff, assisted by Mr. T. G. Mehlin, the McDonald Observatory probably will be located on a peak in the Davis Mountains of Texas, where visual conditions are unsurpassed and the proportion of clear nights is much higher than at Williams Bay.

The 80-inch reflector is being designed at Yerkes by Dr. Struve, assisted by Drs. Van Biesbroeck, Ross, Moffit, Morgan and Crump. It will be surpassed in size only by the 100-inch reflector of the Mt. Wilson Observatory, and will be as useful as that instrument

for the photography of faint nebulae and distant universes.

The astrophysical problems which will be studied at the McDonald Observatory include those of the chemical composition of the atmospheres of the stars, the properties of matter exposed to temperatures ranging from 3,000 to 50,000 degrees or more, observation of distant universes and the study of gaseous nebulae, comets and planets.

No diminution of effort at Yerkes Observatory is contemplated. Dr. Struve will spend approximately half his time there, and the staff will continue its research with the 40-inch refractor. The photographs obtained at the new McDonald Observatory will be studied at Yerkes by the resident staff.

In planning the agreement, the two universities had the advice and support not only of Dr. Struve and Dr. Henry Gordon Gale, dean of the physical sciences division at the University of Chicago, but of many leading astronomers. Dr. George E. Hale, organizer of the Yerkes Observatory and its first director, now honorary director of the Mt. Wilson Observatory, and Director-emeritus Edwin B. Frost, of the Yerkes Observatory, were among the group.

PRESENTATION OF THE PRIESTLEY MEDAL TO DR. CHARLES L. PARSONS

AT the eighty-third meeting of the American Chemical Society, recently held in Denver, the Priestley Medal was presented to Dr. Charles L. Parsons, secretary of the society. In the absence of the president, Dr. Lawrence V. Redman, the president-elect, Professor Arthur B. Lamb, of Harvard University, made the presentation address. According to the report in *Industrial and Engineering Chemistry*, he said in part:

> In 1907, when Charles Parsons became secretary, the society after thirty-one years of existence had 3,300 members; to-day it has nearly 19,000. In 1907 the publications of the society consisted of the *Journal of the Society* and of *Chemical Abstracts*, which had just then been established by Professor W. A. Noyes. These two journals published a total of 5,325 pages in that year. To-day there are eight journals either supported or sponsored by our society, and they published a total of 22,921 pages last year. In 1907 the total annual budget of the society was $30,200; to-day it is $463,000.
>
> Charles Parsons did not accomplish all of this single-handed, but it is to him far more than to any one else that credit for this achievement must be given. At a hundred, at a thousand points it has been his eager efforts, his sound judgment, his shrewd optimism, his sincerity of purpose, his robust spirit and his unfailing loyalty that have launched our new ventures, have consolidated our gains, and have combated those disintegrative forces, those centrifugal tendencies, that might have shaken our solidarity and sapped our strength.
>
> In this rapid growth of our society, with its diverse membership and its far-flung sections, what was most essential of all was a strong stabilizing and unifying force. That force was Charles Parsons. Blunt with the fearlessness of his Pilgrim ancestors who came to our shores in 1620, shrewd with the Yankee shrewdness fostered by our New England hills, kindly and generous and hospitable from his boyhood spent in sunny Georgia, Parsons, though ever at the storm center of our society, has been now its sheet anchor and now its guiding light.
>
> Charles Parsons in his academic, scientific, professional and governmental activities has a splendid record of achievement, which has already won him world-wide recognition—honorary degrees from our universities, the Nichols Medal of our own society in 1905, appointment as officer of the Legion of Honor and of the Order of the Crown of Italy, honorary membership in the Roumanian Chemical Society and in the Society of Chemical Industry.
>
> But it is not these achievements that we are considering tonight. Charles Parsons's great achievement has been the promotion and development of the American Chemical Society. By this means Charles Parsons, more than any one else of his generation, has promoted the advance of chemistry in our country. It is this great achievement that we are recognizing tonight.
>
> It is then with the deepest satisfaction and gratification that I now hand to you, Charles Lathrop Parsons, secretary, the Priestley Medal of the American Chemical Society.

SCIENTIFIC NOTES AND NEWS

DR. ERNEST J. WILCZYNSKI, emeritus professor of mathematics at the University of Chicago, died on September 14 at the age of fifty-five years.

SIR RONALD ROSS died in London on September 16, at the age of seventy-five years. *The New York Times* says in an editorial article: "Millions in fever-ridden countries bless the late Sir Ronald Ross for his discovery of the malaria parasite in the Anopheles mosquito. Yet to remember him for that alone is to do him an injustice. If ever a man was born out of his time, it was he. An Admirable Crichton, who composed music, wrote poems, plays and novels of distinction, dabbled in higher mathematics and in cosmogony, clearly belonged to the sixteenth century. Whatever he did was marked by the grand manner. Scientific method played its part in his medical researches, but he owed his triumph more to the divination of the poet in him."

PROFESSOR L. MIRA, of Barcelona, has been elected president of the eleventh International Congress of

Psychology, which will be held in Madrid four years hence.

DR. A. S. HITCHCOCK, custodian of grasses in the U. S. National Museum, has been elected a corresponding member of the Argentine Scientific Society.

DR. ERNEST SACHS, professor of clinical neurological surgery at the Washington University School of Medicine, St. Louis, has been elected a member of the German Academy of Sciences at Halle.

THE honorary degree of doctor of science has been conferred by Purdue University on Otto F. Hunziker, who was head of the dairy department from 1905 till 1916, when he became manager and director of research for the Blue Valley Creamery Company. Dr. Hunziker was recently appointed chairman of the committee that now manages the *Journal of Dairy Science*.

THE honorary degree of doctor of science has been conferred by the University of Belfast on Dr. Alfred C. Dixon, professor of mathematics at Queen's University.

AMONG those who received the honorary degree of LL.D. at the graduation ceremonial of the University of St. Andrews were Sir James Frazer, author of "The Golden Bough," and Professor L. R. Sutherland, emeritus professor of pathology at the university.

THE Fauchard Medal, the highest distinction in the field of preventive dentistry, was awarded to Dr. Arthur H. Merritt, of New York, at a luncheon held on September 14 in connection with the meeting of the American Dental Association in Buffalo. The medal, which is awarded annually upon the recommendation of a representative group of dentists, was presented by Dr. Marcus Ward, dean of the University of Michigan Dental School, in recognition of Dr. Merritt's work in the field of oral prophylaxis.

PROFESSOR CHARLES H. STOCKING, of the College of Pharmacy of the University of Michigan, was elected president of the American Association of Colleges of Pharmacy at the annual convention recently held in Toronto.

AMONG retirements from the Weather Bureau are the following senior meteorologists: Norman B. Conger, age 72, total service 41 years; Henry B. Hersey, age 70, total service 41 years; Melvin B. Summers, age 55, total service, 33 years.

DR. EMIL PETER SANDSTEN has been appointed director of the Experiment Station at the Colorado Agricultural College, Fort Collins. He succeeds Dr. C. P. Gillette, who has retired.

MR. JAMES RÖGNVALD LEARMONTH, surgeon to the Mayo Clinic and associate professor of neurologic surgery in the University of Minnesota, has been appointed Regius professor of surgery in the University of Aberdeen, in place of Professor Sir John Marnoch, whose resignation takes effect on September 30.

DR. GEORGE M. CURTIS, professor of surgery in the Graduate School, division of biological sciences, University of Chicago, has resigned to accept a similar position at the College of Medicine of the Ohio State University. Dr. Curtis became connected with the University of Chicago in 1925, when he was appointed associate professor of surgery and associate professor of experimental surgery under the Douglas Smith Foundation for Medical Research.

DR. FRANKLIN HOLLANDER, until recently of the department of physiology of the New York Homeopathic Medical College, has been appointed research associate at the School of Dentistry, Columbia University. He will be in charge of the newly organized laboratory of physical chemistry at the Medical Center.

DR. HENRY J. MASSON, chemical engineering, and Dr. Alfred F. Huettner, biology, have been promoted to full professorships in New York University.

DR. PHILIP LEVINE, of the Rockefeller Institute, has been appointed instructor in bacteriology in the University of Wisconsin.

JAMES COULL, of the University of Aberdeen, has been appointed assistant professor of chemical engineering at Cooper Union, New York City.

DR. HENRY J. FRY, professor of biology at New York University, and Dr. Irving J. Saxl, physicist, formerly associate of Guido Holzknecht, of the Central X-Ray Institute of the University of Vienna, will offer courses this winter at the New School for Social Research, New York City.

PROFESSOR STUART A. RICE, who has been granted a year's leave of absence from the University of Pennsylvania, will be a member of the faculty of the department of sociology of the University of Chicago for the year 1932–33, and will give courses in quantitative sociology and in methods of research in the social sciences.

PROFESSOR MADISON BENTLEY, of Cornell University, addressed the Mayo Foundation at Rochester, Minnesota, on August 25, on "Mind, Body and Soul in Medical Psychology."

DR. JAMES A. TOBEY, of New York City, spoke at a meeting of the El Paso County Medical Society on September 9. He also addressed the convention of the Southwestern Dental Association held in El Paso

on September 7, and was the principal speaker at the dedication of the new milk plant of the Borden Company of Texas, opened in El Paso on September 10.

DR. FRANK BLAIR HANSON, professor of zoology in Washington University, has returned to St. Louis after two years' leave of absence, during which time he has been connected with the Paris Office of the Rockefeller Foundation.

M. W. STIRLING, chief of the Bureau of American Ethnology, has returned from a trip to California, where he examined a number of caves and village sites of the Chumash Indians in the mountains west of Buena Vista Lake. On the return trip Mr. Stirling stopped at Mobridge, South Dakota, in order to visit the excavations being conducted there on an old Arikara Indian village site by Dr. William Duncan Strong.

DR. C. H. TOWNSEND, director of the New York Aquarium, sailed on September 8 for Bermuda as a guest of Mr. Vincent Astor. It is planned to transplant several thousand young kingfish, sea bass and bluefish from the waters of New York Bay in an effort to acclimate them to the warm waters there. The yacht will bring back a collection for the New York Aquarium.

AFTER six months of study and exploration in the original home of the potato in Chile, Peru and Bolivia, and on the island of Chiloe, Dr. H. G. MacMillan and C. O. Erlanson, of the Bureau of Plant Industry, recently returned to Washington with 425 lots of tubers and seed and also a number of other plants of possible value for the United States. This expedition was the second made under the special appropriation by Congress for exploration and introduction of disease-resistant potato types into the United States, the other having been to the highlands of Mexico.

ACCORDING to *Museum News*, S. F. Markham, secretary of the British Museums Association, has resigned. Mr. Markham has undertaken the Australian and New Zealand surveys in the Empire Survey of Museums and will be absent from England for six or seven months.

IT is announced in *Nature* that the following awards for the year 1932–33 have been made by the Salters' Institute of Industrial Chemistry: Fellowships renewed to: D. J. Branscombe, University College, Exeter; H. G. Simpson, East London College; J. L. Sweeten, St. Catherine's College, Cambridge; P. Chisholm Young, Trinity College, Cambridge. Fellowships awarded to: S. C. Britton, Pembroke College, Cambridge; E. H. T. Hoblyn, Imperial College of Science and Technology; R. H. McDowell, Jesus College, Oxford; G. Pearce, University of Birmingham. The Salters' Institute has also awarded 107 grants-in-aid to young men and women employed in chemical works, to facilitate their further studies.

AWARDS for contributions to physical therapy were made at the recent eleventh annual session of the American Congress of Physical Therapy to Professor Arsene D'Arsonval, professor emeritus of physiology, College of France, for contributions in electro-physiology, his application of the high frequency current to the treatment of disease and his brilliance as a teacher of scientific men and physicians; Dr. William L. Clark, Philadelphia, for cancer surgery by means of a special process known as "electro-desiccation" evolved by him; Dr. Frederick de Kraft, New York, for work in the development of the diathermy apparatus, his invention of a special device used in the application of static electricity in the treatment of injuries and his work in perfecting various techniques in treating various conditions by static electricity; Dr. F. Howard Humphris, London, England, for contributions in the field of medical electricity; Dr. John Harvey Kellogg, Battle Creek, Mich., for distinguished early pioneering efforts in physical therapy; Dr. Howard A. Kelly, professor emeritus of gynecology, the Johns Hopkins University Medical College, Baltimore, for contributions in the treatment of cancer with radium and the so-called radio knife; Dr. Gustav Kolischer, Chicago, president-elect of the congress, for contributions in electro-surgery and its application to urology; Dr. Albert F. Tyler, professor of clinical radiology and physical therapy, Creighton University Medical School, Omaha, for applications of radium and electro-surgery in the treatment of cancer.

THE estate left by the late George Eastman has been appraised at $25,561,641, with a gross value of $24,403,748 in personal property and $1,157,892 in real estate. Debts included approximately $3,000,000 in unpaid gifts to European dental dispensaries founded by Mr. Eastman. The value of exempt bequests is $20,790,581. The University of Rochester, as residuary legatee, receives $19,287,143. The second largest beneficiary, the Rochester Dental Dispensary, receives $1,028,438.

ACCORDING to the *Journal* of the American Medical Association, the Rockefeller Foundation recently made available to Stanford University a gift of $200,000 to promote research in the social sciences. This research has been supported for a number of years by the Laura Spelman Rockefeller Memorial Fund. Seventy-five thousand dollars of the total, which is to be distributed over a five-year period, is unconditional.

The remainder will be granted on the condition that the university provide an equal amount from other sources.

THROUGH means of a fellowship made available by the Anthracite Institute, investigation of the physical and chemical characteristics of anthracite coal will be carried on at the Pennsylvania State College during the coming year, according to announcement made by Dr. A. W. Gauger, of the School of Mineral Industries. G. Lum, a graduate of Penn State in 1925, has been named to conduct the work. The actual behavior of coal under burning conditions will be the specific objective of the researches.

MRS. ESTHER TIMBRELL, of Rutherford, New Jersey, has given to the department of zoology of the New Jersey College for Women, Rutgers University, for display and study in the work in ornithology, a collection of some 200 mounted birds, including rare species, which makes the collection now housed in the Federation Hall Museum of Zoology virtually complete as regards the native birds of the state. With this collection came also some 50 exotic birds, sponges, corals, and several skulls and other skeletal parts, including the skull of a walrus taken on one of Peary's expeditions in search of the North Pole.

THE New York State Department of Education has listed for the guidance of students wishing to study medicine abroad the medical schools that meet its requirements for licensing examinations. The approved schools in Canada are those of Dalhousie University, Halifax; McGill University, Montreal; Queens University, Kingston, Ont.; Laval University, Quebec, and the universities of Toronto, Alberta (at Edmonton) and Western Ontario (at London). Medical faculties of all governmental universities in Austria, Germany, Holland, Hungary and the Scandinavian countries and all those that are part of English, Irish and Scottish universities are on the list. In order that courses in medicine in these foreign schools may be accepted, it is necessary that the students take two years of premedical work, including the required sciences, before beginning professional study.

THE organization of a committee of engineers, architects, physicians, builders, health officials and others, under the auspices of the American Standards Association, to establish authoritative national standards for the ventilation of buildings, has been requested by the American Society of Heating and Ventilating Engineers. The society's request, made public on September 15, is the result of a growing conviction among architects and engineers that the municipal ventilation codes now in force throughout the United States are based solely on tradition and are without scientific foundation. In this connection the American Institute of Architects recently adopted a resolution demanding the overhauling of all existing municipal ventilation codes. As a starting point for the proposed work, the Society of Heating and Ventilating Engineers submitted the recommendations of its Standards Committee, based on a comprehensive research program on the effects of ventilation on health and comfort still in progress at the society's research laboratory in Pittsburgh. If these recommendations are adopted, one important result will be a lowering of fuel bills through a reduction in the amount of outside air required by present codes to a minimum of 10 cubic feet per occupant per minute. The recommendations cover temperature, humidity, air quality, movement, distribution and quantity.

ACCORDING to the *Journal* of the American Medical Association the United States is the only manufacturing country which has thus far ratified the treaty drawn up at the international conference on limitation of the manufacture of narcotic drugs, held in Geneva in 1931 under the League of Nations. Nicaragua, Persia, Peru and Portugal, all non-manufacturing countries, have ratified the convention. Unless twenty-five countries, including four manufacturing countries, have ratified the convention before April 13, 1933, it can not come into force. Other manufacturing countries are France, Germany, Great Britain, Japan, The Netherlands, Switzerland and Turkey.

BY act of Congress approved on July 14 the District of Columbia is, in effect, made a bird sanctuary. The act repeals a provision of law which allowed shooting of wild waterfowl on parts of the Eastern Branch and on parts of the Virginia side of the Potomac River within the district. The administration of this law, as in the case of most other laws affecting the district, comes under the metropolitan police, but the game protector of the Bureau of Biological Survey stationed in Washington will cooperate with the police. Disturbance of the birds by gunning lower down the river is likely to have a tendency to concentrate them in district waters. The opportunity to see these birds from automobile roads is one scarcely equaled in any other large city of the country. Living either for a time or permanently in the midst of a bird sanctuary will enable the people to become more intimately familiar with the birds and the desirability of protecting them as a natural resource.

THE London *Times* reports that a replica of a map of the polar regions, prepared by Professor N. N. Zooboff, of the Arctic Institution, is reproduced in miniature on stamps provided by the Soviet Government for the purpose of a special air mail to be dispatched by the Second International Polar Expedition

from Franz-Josef Land to Archangel by way of Nova Zembla this month. To the left of the map, in an upright panel, appears a picture of the Russian ice-breaker *Sibiriakoff* in the Arctic Sea, with an aeroplane flying overhead, while on the right is seen the Soviet emblem of the hammer and sickle. Designed by J. J. Doobassoff, and engraved and printed at Moscow to the extent of 10,000 copies each, the two stamps in denominations 50 kopecs red (for postcards) and 1 rouble green (for letters) illustrate the results of the latest discoveries made in cooperation with the airship *Graf Zeppelin* last year.

DISCUSSION

SPILLMAN'S WORK ON PLANT BREEDING

ON July 11, 1931, it was announced that Dr. W. J. Spillman, a leading agricultural economist, had died. Little emphasis was placed on the outstanding work that he did as a plant breeder at Washington State College, Pullman, Washington, from 1894 to 1901. Here he laid the foundation for the development of some highly desirable commercial wheats, such as Hybrid 128 and Hybrid 143. He also independently discovered the two chief tenets of Mendelism; that is, the combination and segregation of plant characters in hybridization.

Dr. Spillman was educated at the University of Missouri, where he earned the B.S. and M.S. degrees and by whom he was honored with the degree of doctor of science in later years. He spent some of his early days teaching in Oregon and was finally called to the important post of crop specialist at Washington State College, in 1894. At first he was interested in forage crops and grew as many as one thousand different varieties. Later, he saw that wheat was the most important crop and offered the chief problems. His first method of attack was to get out a questionnaire to the growers. The interest of the growers was indicated by the replies, which numbered 143. These facts, together with the ones which followed, were given by Dr. Spillman himself in an interesting and enlightening talk before the writer's class in advanced crop breeding at Oregon State College on January 29, 1931, just about six months before his death. These facts, I am sure, will be of great interest to all agriculturists in the Pacific Northwest and all interested in heredity, for they show the very able beginnings of a long line of valuable wheat-breeding results obtained by the Washington Experiment Station.

The questionnaire which Dr. Spillman sent out to the wheat growers of eastern Washington revealed the fact that the chief varieties grown were Little Club, Bluestem and Red Chaff. Little Club was grown where there was the most rain; Red Chaff where there was a somewhat smaller amount of rainfall; and Bluestem where there was the least. Dr. Spillman was impressed with the fact that all these were spring wheats, but that they were chiefly grown in the fall and that two thirds of the wheat area was fall-sown wheat. All the growers were interested in varieties which were more winter hardy. They stated that good yields were obtained as long as winter killing was not a factor. Dr. Spillman began, therefore, a search for winter wheat varieties, and made contacts with all experiment stations in the United States and other countries for wheats to use in his breeding program.

One spring a field comprising 10 or 15 varieties of wheat in series was used for a summer pasture. These varieties were sown in the spring. Part of these headed and part did not. He learned the difference by this incident between the winter and spring varieties. At this time leading authorities did not know that there was a difference between winter and spring wheat. In April of the following year 90 winter varieties were sown; 30 headed. These were mostly varieties from the Southern states. Thirty winter types were saved. The other 30 varieties were mixed winter and spring wheats. Wheat with strong straw yielded well. The standard plat used was 1/60 of an acre. This size plat allowed him to take pounds per plat equaling bushels per acre. Five of such wheats were planted for increase. It so happened that rain and wind at heading time flattened out these five varieties of wheat. This probably saved distribution, as it was intended to increase these varieties for circulation.

Later, Dr. Spillman got suggestions from Dr. W. M. Hays, plant breeder at Minnesota, who suggested crossing of varieties, and Dr. C. V. Piper, then at Pullman. At this time it must be remembered that Mendelism was not yet known. The idea of crossing Turkey and Little Club occurred to Dr. Spillman. It was hoped that a combination between the winter hardiness of Turkey and the strong straw and the tight, non-shattering heads of Little Club might be obtained. Eleven crosses were made involving these and several other varieties. Red Chaff and Little Club were parents in all these eleven crosses, at least on one side. Three hundred three kernels were obtained which were thought to be hybrids. The grains were planted on 6″ intersecting lines. Seed was fall planted and 303 plants were produced. It was

thought by fall planting that the winter would kill out all but the most hardy individuals, which were the ones desired. During the winter the rain cut a gully a foot wide and a foot deep right through the patch of hybrids; however, most of the plants were saved. Heads of the parent varieties were saved for comparison. It was found that there were 169 real crosses left after the plants were grown. Each plant was pedigreed separately, a nursery was laid off in ranges, and plant families were begun in the F_2 generation.

The earliest plants found were from crosses between Red Chaff × Valley. The segregations of other varietal crosses were noted particularly where Turkey was one of the parents, because of the beards. About this time Dr. Spillman was sunburned from being in the field so much studying hybrid plants and had to go to the hospital. During this period, E. E. Elliot took charge. Twenty-two types of plants were obtained in the F_2 of his cross of Red Chaff × Valley.

A talk was given to a teachers' association at Pullman, and they were told by Dr. Spillman that the 22 types of wheat were all the kinds of wheat in the world, as he thought this to be true at that time. Segregation was noted and also the recombination of characters which had originally been present in the parent varieties.

In 1901 a paper was prepared and read in Washington, D. C., describing the F_2 generation of the crosses. Segregates from different crosses were mounted on cloth, and the F_{1s} and F_{2s} of all eleven crosses were shown. At the lecture Dr. W. M. Hays was on a front row seat and was much surprised at the results shown. Dr. Spillman emphasized recombination and segregation of characters in plants rather than a general blending. It is interesting to note that the papers of DeVries, Tschermak and Correns were published in January, February and March of 1901, in the months preceding Dr. Spillman's work. His discovery was made in August, without knowledge of the earlier work.

About this time and after presenting his paper in Washington, D. C., Dr. Spillman was offered a position there and left Pullman. Continuation of the work of selecting desirable strains from the crosses was left to Mr. Elliot and those who succeeded Dr. Spillman. Spring wheats were discarded, since the winter wheats were the types desired. After four years, Hybrid 128 and Hybrid 143 were isolated. Hybrid 128 has long been a standard variety in the Pacific Northwest, where the rainfall is over 13½ inches.

Thus, although not responsible for the selection of the segregates which later proved to be valuable varieties, Dr. Spillman was the one who planned and began this work and in doing so made an important contribution as a plant breeder.

E. N. BRESSMAN
OREGON STATE AGRICULTURAL COLLEGE

AN OBSERVATION ON THE LONGEVITY OF SERRATIA MARCESCENS (B. PRODIGIOSUS)

ON April 24, 1932, the writer had the opportunity to examine certain old stock cultures which had belonged to the late Professor H. H. Waite. The oldest of these cultures had been made in 1903, and other cultures ranged from that date up to 1911. All these cultures had been sealed by means of sealing wax.

The media upon which the cultures had grown consisted for the most part of nutrient agar and nutrient broth. The cultures had been stored in the dark at room temperature. Cultures which showed evidence of drying due to breaks in their seals were not examined.

A broth culture which had been inoculated on November 1, 1909, with *B. prodigiosus* (now known as *Serratia marcescens*) was chosen for examination. The wax seal was removed with the aid of heat and the cotton plug taken out. Using a sterile platinum wire loop, one drop of the culture was distributed to each of the following media: standard nutrient broth, standard nutrient agar (liquefied and held at 45 degrees Centigrade and later poured into a sterile petri dish), and "K" medium. All cultures were incubated at room temperature.

At the end of a 24-hour incubation period all the cultures showed visible signs of growth. The petri dish culture showed three colonies about 3 mm in diameter with a slight pinkish tinge at their edge. After longer incubation the colonies took on the deep red metallic sheen of fuchsin and when touched with the platinum wire exhibited the characteristic ropy test. All the other cultures proved to contain *Serratia marcescens* in pure culture.

The observation seems then to have shown that an organism of a non-sporogenous type can remain viable in a fluid medium, in the presence of its own metabolic waste products, and at a reduced oxygen tension, for at least a period of twenty-two and a half years.

WILBUR E. DEACON
UNIVERSITY OF NEBRASKA

POINTERS FOR STARS

IT is difficult to point out to another person a particular star or constellation when the night heavens are being viewed without instrumental aid. If a fairly powerful long focus flash-light, such as can be pur-

chased for a few dollars, is directed at the night sky it can be used as an effective pointer. The beam loses itself at a sufficient height to make the pointing unambiguous to a small group. This use of a flash-light recalls the use of an electric light pointer by the lecturer in a Zeiss planetarium.

WATSON DAVIS

SCIENCE SERVICE,
WASHINGTON, D. C.

SPECIAL CORRESPONDENCE

CANADIAN PARTICIPATION IN INTERNATIONAL POLAR YEAR

IN view of the great extent of Canadian territory in high latitudes, it is natural that Canada should take an active part in the activities of the second International Polar Year. In this great international enterprise, intensive observations according to a uniform plan will be made at a large number of Arctic and sub-Arctic stations in terrestrial magnetism, meteorology and aurora. Observations in these subjects, which began soon after August 1, 1932, will continue until August 31, 1933.

The Canadian Meteorological Service has been in charge of the organization of the Canadian program. Parties will occupy three stations in Northern Canada, in addition to extending materially the magnetic observations now being taken at Meanook, Alberta.

The largest party, consisting of F. T. Davies, formerly magnetician of the Byrd Antarctic Expedition, B. W. Currie, assistant professor of physics, University of Saskatchewan, S. T. McVeigh and John Rea, will go to Chesterfield Inlet, latitude 63° N., longitude 90° W., on the northwest shore of Hudson Bay. Chesterfield Inlet is about 475 miles from the north magnetic pole and is the nearest point to the pole at which continuous magnetic records will be taken during the "International Polar year." Using a set of the recently designed laCour magnetographs, three continuous records will be obtained of each of the magnetic components—horizontal and vertical magnetic force and declination. With an equipment loaned by the Department of Terrestrial Magnetism, Carnegie Institution of Washington, continuous records will be taken of the feeble electric currents which flow through the earth's crust, and which have been found to vary closely with magnetic disturbances.

It is planned to photograph the aurora, simultaneously at two stations about 30 km apart, using the Krogness f. 1.25 auroral cameras. From these photographs the height of the aurora may be determined. Using the McLennan night sky spectrograph, it is hoped that the spectra of all aurora will be obtained, and thus establish whether aurora of a certain spectral type are associated with auroral displays at definite elevations above the earth's surface.

The meteorological program, in addition to intensive routine observations, includes daily kite ascents, pilot balloon observations and temperature data to a level of 3 km or 4 km from the Patterson light-signalling meteorographs. The kite equipment both for Chesterfield Inlet and for Coppermine has been loaned by the U. S. Weather Bureau.

Records of the atmospheric electric potential, nuclei of condensation and of the temperature at the top of the radio mast, 45 meters high, will throw further light on the atmospheric conditions in this region.

R. C. Jacobsen will be in charge of a station at Coppermine, on Coronation Gulf on the Arctic Ocean. The supplies and equipment for this remote point have been forwarded by steamer from Vancouver *via* Behring Strait, but the observer in charge and his assistant will be flown in by airplane from Northern Alberta. The program at Coppermine will be devoted to meteorology and auroral observation. An interesting feature of the meteorological work will be an attempt to determine the height of the stratosphere by the grid type of Moltchanoff meteorographs. These instruments emit automatically every few seconds radio signals giving the temperature and pressure of the point aloft at which the meteorograph is situated. These signals, which resemble a Morse code, are picked up by a receiver on the ground, and from them the true temperature and pressure are determined. The Moltchanoff meteorographs have been donated by the Rockefeller Foundation on the recommendation of the International Polar Year Commission.

At Coppermine on the bleak Arctic coast line the temperatures of the air up to 3 km or 4 km will be taken at frequent intervals by kite and light-signalling meteorographs. Both the Chesterfield Inlet and Coppermine parties will use the Robitzch actinometer to record the solar radiation, while at Chesterfield Inlet readings of the kata thermometer will be taken thrice daily.

A third station at Cape Hope's Advance, Latitude 61° N., Longitude 70° W., on Hudson Strait, will be operated by J. E. Lilly, with the assistance of the operators of the radio station here. The program will be meteorological and auroral, similar to that at Coppermine, but not as extensive. Cape Hope's Advance will serve as an effective link between the Danish and French stations on Greenland and the réseau of American stations.

Supplementing the data taken at Chesterfield Inlet,

Coppermine and Cape Hope's Advance, a group of 15 northern stations will report the occurrence of aurora, giving its intensity in terms of the brightness of the auroral green line in a direct vision spectroscope.

From the data obtained at "polar year" stations in both eastern and western hemispheres, one may expect information that has been needed for years in geophysical science. The movement and characteristics of polar air masses, their interaction with air masses in temperature latitudes, the simultaneity of auroral forms, and magnetic disturbance over the earth are examples of large-scale problems which could only be treated by each nation contributing a share in a world-wide endeavor.

ANDREW THOMSON
METEOROLOGICAL SERVICE OF CANADA, TORONTO

QUOTATIONS

HOW A NEWSPAPER LOOKS AT COSMIC RAYS

THE *New York Times* for September 18 prints a leading editorial article entitled "Cosmic-Ray Romancing" which reads as follows:

Your physicist is supposed to be a hard, matter-of-fact measurer who suppresses romantic speculation and talks only of energy, volts, ions and electrons. Confront him with a mystery and he proves to be as human as the rest of us. Consider the cosmic rays. For years Millikan in this country and Kolhoerster, Hess, Regener and others in Europe have been studying them only to their own mystification and ours. Measuring instruments are dropped into lakes a thousand feet deep or elevated twenty miles above sea-level. Piccard imperils his life to determine the true nature of the rays. Professor Compton and a devoted band of physicists station themselves at the Equator, in the far north, on mountaintops, in deep mines to conduct their investigations. And the result? Romance—sheer romance.

Millikan spins a tale of electrons and protons combining in space, and of resultant cosmic rays that proclaim the continuous upbuilding of the universe, contrary to all the laws of thermodynamics. Jeans holds us spellbound with a poem about stars dying in a fierce radiance and bombarding us with cosmic rays in the process. Regener, as practical as the Irish foreman of a railway section gang when it comes to counting ions, looks at his equations as into a crystal and sees the beginning of things—sees primitive stars shedding cosmic rays and suffusing a relativistic universe from which they can not escape because it is closed and finite. Stimulated by him, others imagine that, just as the bones of a dinosaur tell us something of the life that was on earth a few million years ago, so these fossil cosmic rays reveal the Almighty in the act of fashioning electrons and protons into nebulae, suns, planetary systems and man himself.

For all the instruments and methods invented to test the cosmic rays, the physicist is still the medicine-man from whom he is descended. Electroscopes and ionization chambers and other cosmic-ray measuring instruments seem strangely like wands and totem poles, and Einsteinian equations but incantations that make us believe we know more than we really do. That we are actually dealing with something like wish-fulfillments in the cosmic rays is evidenced by the results obtained. Here is Millikan convincing himself that the cosmic rays prove that the universe is self-perpetuating. And Compton, adopting precisely the same methods, reaches the conclusion that the rays are only electrons swerving to the Poles because the earth is a great spinning magnet. What are the cosmic rays? There is no positive answer. We simply try to reconcile what the instruments indicate with our hopes and beliefs and imagine we understand the cosmos.

The same issue of the *Times* contains an editorial note entitled "It is Done with Mathematics" which reads:

It is a relief to read that Professor Compton is back from studying cosmic rays in the Arctic region with the definite report that Professor Millikan is wrong. The cosmic ray, says Professor Compton, is not a wave, as Millikan thinks, but a particle.

It is a relief to find that when two men in the high realms of science hold opposite views one of them is right and the other is wrong. Hitherto the public has had to get used to the idea that when two great physicists differ radically about something in the universe the answer is that both men are right.

What is the electron, a wave or a particle? It spreads after going through a hole, like a wave. It hits other electrons like a particle. Easy, says science. An electron is both a wave and particle. That would be nonsense by the rules of common sense, but it makes sense in the new sciences. There is a formula for it.

Some people think the universe is expanding. Some people think the universe is contracting. They are both right, says science. Professor Eddington can think of its being an expanding universe and a contracting universe simultaneously. Or, rather, he can find a mathematical formula that will describe that startling situation.

In the same manner space is finite and space is infinite. There is a formula.

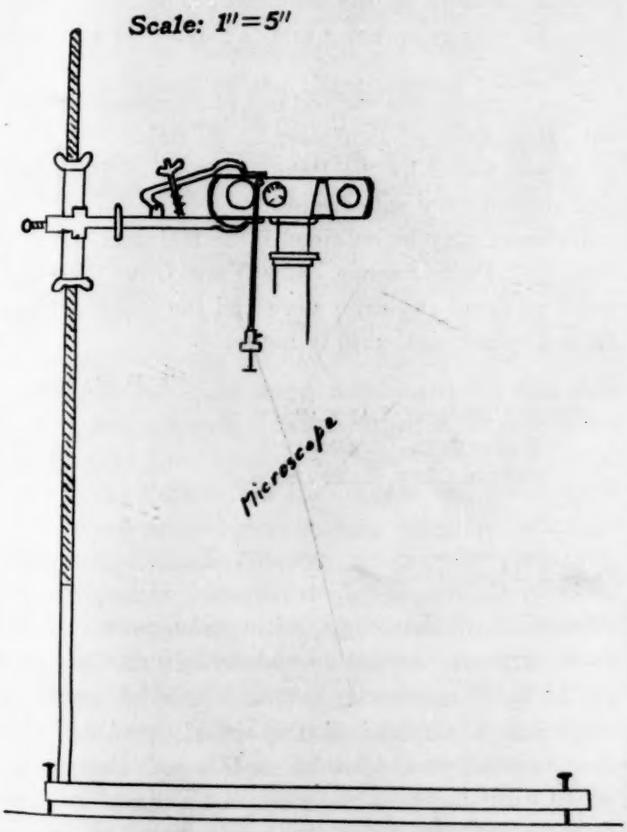
Obviously it is a delightful world in which you can have the coffee simultaneously hot and iced and out of the same cup, your egg simultaneously hard-boiled and scrambled, and the griddle cakes at the same time round and oblong.

But occasionally it is a relief to find black as the opposite of white and right as the counterpart of wrong.

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A SIMPLE APPARATUS FOR TAKING PHOTOMICROGRAPHS

WHEN making photomicrographs much time is involved in setting up the apparatus preliminary to making the exposures. Hence it is often inconvenient to take just two or three pictures. The writer has devised a stand for supporting a small camera over a microscope. The camera used is a small one made by a well-known German manufacturer of microscopes and other optical equipment. It uses standard size motion picture film and has a fast lens that enables the operator to take photomicrographs with very short exposures. The support here described



permits one to set up the apparatus in about two minutes.

The stand for supporting the camera over the microscope consists of a steel base, 15 inches long by 6 inches wide, drilled and threaded at each corner to take a thumb-screw. These are used for leveling. At one end, midway between the two thumb screws, an upright from an ordinary laboratory ring-stand is fitted into a threaded hole in the base. This upright is threaded from the top to within about 5 inches of the bottom. A wing-nut is screwed on to the upright, a loose fitting three-inch section of brass tubing is placed over the upright and on top of the first nut, then another wing-nut is screwed down on to the brass sleeve. One end of a short burette clamp is attached to the middle of the sleeve and the camera secured at the other end, lens down. By loosening the lower wing-nut and tightening the upper, the camera may be lowered and clamped firmly at any desired elevation.

The microscope is placed directly under the camera and the latter lowered until the rim of the metal mounting of the lens is in contact with the mounting of the eyepiece of the microscope. With the particular camera and petrographic microscope used, the ocular of the microscope fits snugly within the raised edge of the camera lens mounting.

Since the human eye at rest is focused at infinity, it is unnecessary to focus on the object with the camera; the camera is simply raised a bit, swung aside, the microscope focused on the object to be photographed, the focusing screw of the camera set at infinity, the lens opening set at f.3.5, and the whole swung back and lowered into place over the microscope. Of course the length of exposure will have to be determined according to existing conditions.

The accompanying figure is self-explanatory.

MARCELLUS H. STOW
WASHINGTON AND LEE UNIVEPRSITY

A METHOD FOR THE PRESERVATION OF BOOK BINDINGS

IN an earlier communication[1] a method of preserving manuscripts was discussed. In the present paper is described a continuation of the work to the protection of book bindings, etc. The dressing of the British Museum employs beeswax dissolved in hexane. We have used a wax which has advantages over beeswax and a solvent which is cheaper and more readily obtainable.

A solvent is made of equal volumes of carbon tetrachloride and benzene and kept in a tightly stoppered bottle to prevent evaporation. The carbon tetrachloride makes the solution less dangerous in the presence of fire. However, the solution should be kept away from flame.

To one liter of the solvent mixture is added 100 grams of Halowax No. HXRD–2–25. The wax is allowed to dissolve in the solvent. Halowax is a commercial product with a melting point of 252° F. It is a flexible material, and the melting point is far above the highest temperature reached during summer

[1] Sanders and Cameron, SCIENCE, 74: 1924, November 13, 1931.

weather. After the halowax is dissolved, 200 grams of anhydrous lanolin is dissolved in the mixture. If hydrous lanolin is used, about 300 grams is required, since hydrous material contains approximately 35 per cent. of water. In case anhydrous lanolin can not be obtained and the hydrous material is used, after solution has taken place a layer of water mixture collects at the top, on standing. The lower layer should be separated by use of separatory funnel. After the halowax and lanolin are completely dissolved, 50 ml of cedarwood oil is added. This solution should be kept in a tightly stoppered bottle.

The solution may be applied to the book bindings as a spray, but we have found it preferable to use a camel's-hair brush. Care is taken that a uniform coating is applied to the book. The book is held or placed in open position until dry. The solvent mixture dries quickly, since the solvent components have a high vapor tension. When dry, if too much wax appears in places it may be smoothed by the brush dipped into the solvent.

Carbon tetrachloride and benzene can be obtained on the market cheaply and in pure form. A mixture of the two in equal volume forms a solution that is not explosive. However, reasonable precautions should be observed. This solvent, aside from being cheaper and easier to obtain, forms a more homogenous solution than does hexane. The halowax used has a higher melting point and gives a better polished surface than beeswax.

The lanolin gives the oil protection to the book bindings, especially in the case of leather bindings. The lanolin does not become rancid on standing. The quantities of halowax and lanolin may be varied, depending on the particular binding to be treated.

This treatment protects the book from the atmosphere, the halowax and lanolin forming a waterproof film after the solvent has evaporated. Any breaks in the film due to bending may be smoothed by brushing with the solution.

The halowax itself is an efficient repellent towards fungus diseases and towards insects and rodents. In case that further treatment with insecticides or fungicides is desired, these may be added to the solution (provided they are soluble in the solvent) and applied at the same time.

Book bindings so treated can be cleaned and washed without damage to the bindings. The coating produces no change in color and the dye does not "run" in the solution.

This formula was worked out at Dartmouth College with the advice of Professor C. E. Bolser. The formula was tested by the Baker Library of Dartmouth and proved very satisfactory.

Halowax may be obtained from Halowax Corporation, 247 Park Avenue, New York City, the cedarwood oil from any drug store and the other materials from any chemical supply house.

J. P. SANDERS

STEEL CHEMICAL LABORATORY OF
DARTMOUTH COLLEGE,
HANOVER, NEW HAMPSHIRE

SPECIAL ARTICLES

WOOD OPALIZATION

ABSENCE of petrified dicots in older Mesozoic sedimentaries, though many other forest types, either calcified or silicified, occur the world over from Paleozoic times down, even twenty-five years ago seemed primarily due to non-existence of the dicots. Then the antiquity of flowering types began to come into view; and presently Dr. Marie Stopes was able to describe four well-defined genera of dicot stems (partly calcified?) from the lower Greensand. Nextly, Kraeusel reported a further type from the weisser Jura of Germany. While a Trochodendron-like stem fully silicified from the mid-Jurassic of India has just been described by Sahni.

Valid reasons for a sparsity of dicot stems in many pre-Tertiary horizons are purely physio-chemic. In lignites, due to easy liability to bacterial decay, dicots are often found much broken down where conifers still retain their finer structures. In accord the beds of coal are made up of decay-resistant types. Also, chalcedonized logs, with coaly centers or surfaces, are often noted in the field; and since much partly lignitized, partly silicified material occurs, the suspicion is emphasized that in all probability, in fresh-water beds at least where change is slow as compared with what goes on in eruptives, it must be woods in the lignitic condition that are replaced by silica or calcium carbonate. The lignitic or lightly carbonized stage once passed, there is little further replacement, else there should be notable silicified bands in bituminous and anthracite coals, in the various forms of durain and vitrain. Such are quite unknown.

Evidently siliceous or calcareous replacement depends on what are termed "reversible" chemical reactions; and once log rafts reach the lignitic condition any abundance of lime with calcitic deposition and some desiccation must be peculiarly favorable to a later course of opalization. Filling in of desiccated lignites by sheets of calcite was seen by Brown in one of his Pleistocene mammal quarries in Eastern

Cuba; and such a condition surely occurred in the early course of petrifaction in the superb Dresden cycadeoid Raumeria, being also indicated in stems of the Black Hills Como.

Calcification direct, however, seems precluded in eruptives or mainly ensues under delta or coastal marine influences and conditions, as in "coal balls," in the palms of the Pierre, and notably at the Wealden "log raft" of the Isle of Wight, or on the coast of Australia, as recorded by Mantell. In 1843 Hugh Miller saw the farmers of the Sutherlandshire coast at Helmsdale quarrying and burning (!) for lime the large annular ringed calcified logs of the Oölite. These are mainly conifer forests.

Apparently, where silicified dicots occur at all in fresh-water sedimentaries, their presence has been in some way favored by unusual conditions or influenced by nearness to epicontinental sea-levels or waters. The fossil forests of the Cairo region, known for a hundred years, may be cited; while the dicot forest with Lauraceous types which I found along the Chuska Mountain front of New Mexico may not be an exception. This is the finest older petrified dicot forest thus far found in North America.

Nevertheless, as noted in the Carnegie Institution Year Book for 1930, monosilicic acid thermal in source must account for certain petrified forests; and corollary to the slight decay resistance of dicots, their most typical occurrence is in eruptives, as seen along the slopes of the "Specimen Ridge" above the Lamar River in the Yellowstone Park. Much of the petrified dicot record must hence be lost, since the eruptives which would contain it are, as geologic time goes, most evanescent features; though it could be admitted —is in fact apparent—that, during the post-Jurassic course of both dicot and monocot stem evolution, decay resistance has been somewhat increased. Certainly the palms are freely petrified in sedimentaries after lower Cretaceous time. Albeit in later courses of volcanism, with the decay factors eliminated, and in the presence of both heat and abundant soluble alkaline silicates, the dicots reach as free silicification as conifers, and both are found in the more and more opaline condition. What may be the most recent examples of wood opalization can not have been very well reported, though Pleistocene woods freely occur; while so far as casual observation goes the pre-Miocene forests generally have lost their water content and reach solid chalcedonization.

Nearly all my own earlier field contacts were with the Mesozoic sedimentaries, and there the greater record seemed to lie. But once I became aware of the opalized dicot-conifer record it seemed amazing. It is in fact a record but superficially described. The opaline forests of the eruptives were long just examples of opal to the student of orogeny, indicating earlier thermal conditions and replacements; while in this country at least those botanically informed seldom studied first-hand the occurrences along the remoter mountain slopes. Dependence of opalization on the decay factor got as little attention as the rate of replacement and water loss in the several types of wood, surely a subject of sheer interest.

Time factors being thus undetermined, and observations such as those of replaced pine needles in the kettles about the Yellowstone Lake merely casual, attempts made in laboratories from time to time to mineralize wood have hardly reached dignity of record in chemical journals; while no great industries being dependent on the silicon series, it is still relatively unstudied. Impregnations or siliceous sinter coatings are the merest first stages in wood opalization. Nor would it be a practical scientific solution of the greater problem to stop short of analyses and complete replacements. The nature of siliceous gels, their solubilities, the fact that the action of the soluble alkaline silicates on cellulose is hastened by heat and probably by inequal pressure accelerating capillary flow through impregnating wood, has been evident enough. Long ago Church silicified corals in the laboratory. Nevertheless, the years have gone on without specimens of artificially silicified woods appearing in the laboratory. Such, when they do come, to be convincing should be brought at least to the clear opalization stage, and should have size enough to in some measure determine possible use in the arts.

The brilliant colorings of the opalized and chalcedonized woods of the lavas and ash are always a source of wonder, though long reproduced artificially by means of well-understood technique. But what must astonish much more from any chemico-physical point of view is the even light coloring of large silicified or opalized logs, indicating with dissolving out of the last traces of cellulose a reaction in an abundant and uncolored gel as steady and as well defined chemically as even the segregation of a quartz crystal or a sphere of hyalite. In a study to be brought out by the Field Museum of Chicago I have given what is trusted may prove a needed and readable general account of petrified forests, with adequate references to the literature concerned.

G. R. WIELAND

CARNEGIE INSTITUTION

ON THE BEHAVIOR OF A GLASS PLATE FLOATING NEAR THE EDGE OF A FLAT-TOPPED DROP OF MERCURY

IT does not seem to have been noticed that while a thick plate of glass floated on the top of a flat drop of mercury is thrown off by the capillary forces, if it

approach the edge, a thin plate, under the same conditions, has a position of stable equilibrium near the edge of the drop. This seems to depend on the constancy of the angle of contact between the glass and the mercury.

Let $A\ B$ (Fig. 1) be the section of a flat drop of mercury cut by a vertical plane, and let $C\ B$ (Fig. 1)

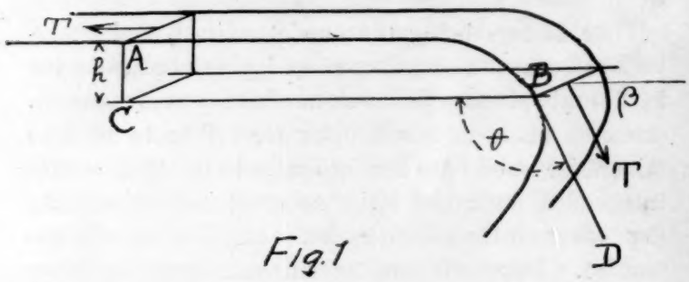


Fig. 1

be a horizontal surface at such a depth that the angle $C\ B\ D$ (θ) is the angle of contact of mercury with glass. Consider the equilibrium of the portion of the drop contained between the planes $A\ B$, $B\ C$ and a plane parallel to $A\ B\ C$ but unit distance from it. If T be the surface tension, and D the density of mercury, and β the angle to the horizontal of the mercury surface at B,[1] we have for the equilibrium of this portion of the drop since θ is greater than 90° (horizontal components)

$$\frac{h^2}{2}Dg - T + T\cos\beta = 0 = \frac{h^2}{2}Dg - T - T\cos\theta.$$

The forces due to atmospheric pressure cancel out.

If a thin piece of glass (say 1 mm thick) be placed on the mercury surface and loaded until it displaces the fluid within the planes specified above, it will rest in equilibrium; for, by Poynting's lemma[2] the horizontal forces on the glass will be the same as if the fluid were not depressed but ran at its level right up to the vertical surface of the glass.

Now, let the load be increased so that the glass sink a further distance h'. The glass at first takes the position shown in Fig. 2, where the resultant hori-

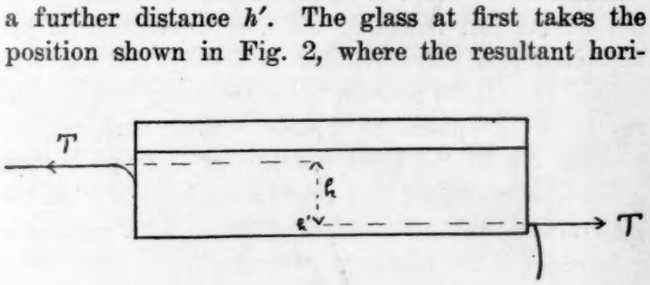


Fig. 2

zontal force is to the right, and is due only to the difference in the hydrostatic pressures $(h+h')\ Dg$ and $h'\ Dg$. As soon as these forces have moved the glass so that it projects from the mercury, we have the state of affairs shown in Fig. 3, where, since θ and T are

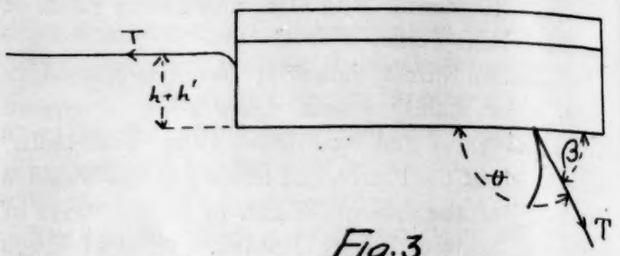


Fig. 3

the same constants as in our equation above, the increased hydrostatic pressure drives the glass off the flat surface of the drop. But, if the load be lessened, so that the lower surface of the glass rises above the level of $B\ C$ in Fig. 1, to depth $\bar{h}$, the fact that T and $T\ cos\ \beta$ have not altered and the term due to the hydrostatic pressure is less, there remains a resultant force that drives the glass to the left. As the line of glass-mercury contact comes to the corner where the glass surface bends sharply upwards, the angle $\bar{\beta}$ (to the horizontal) rapidly diminishes (θ remaining the same) and we get a position of equilibrium where

$$\frac{\bar{h}^2}{2}Dg - T + T\cos\bar{\beta} = 0.$$

If the glass be pushed either way from this position the resulting change in $\bar{\beta}$ gives a resultant force that drags it back to the position of stability.

The behavior of the thin glass plates in Manley's manometer[3] seems to depend on this phenomenon.

WILL C. BAKER

PHYSICAL LABORATORY,
QUEEN'S UNIVERSITY,
KINGSTON, ONTARIO, CANADA.

[1] Note that while $\theta + \beta = 180°$ here, β but not θ changes in the last case considered below.

[2] See Poynting and Thomson, "A Text-Book of Physics. (*Properties of Matter*)," Chap. xiv, p. 153.

[3] *Proc. Phys. Soc.*, London, Vol. xl, p. 178, June, 1928.

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